

peculiar tint while producing the test colour of the strychnine, and consequently in no way interferes with the result.

Upon the capabilities of this adjustment the real value of the bichromate depends. It is obviously impossible for us to give the precise proportions of sulphuric acid and bichromate of potash (chromic acid will do quite as well) for the quantity of strychnine; but every operator, by experimenting with solutions of strychnine of known and diminished ratios, will soon learn for himself.

We have stated that the solution of strychnine should be in chloroform; for while, with the precautions and manipulations that we have published, we believe that preference will almost universally be given to this fluid over ether, benzole, &c., for the purpose of *first* separating strychnine from the subject of analysis, still we readily admit that the fluid chosen for this purpose is not of such paramount importance as that the solution of this poison finally prepared for treating should be in chloroform, for the simple reason that, while the other fluids, such as ether, have a remarkable tendency to spread over the surface on which they are put for evaporation, and so dilute, as it were, by extension, chloroform, by having a contrary tendency, leaves the strychnine on a smaller space than the fluid occupied at the moment of its transference to the porcelain.

We observe with satisfaction that Dr. Glover, in his letter published in your columns of the 12th inst., has expressed an opinion identical with ours published by you on the 28th ult.

We are, Sir, your obedient servants,

J. E. D. RODGERS, M.R.C.S.E., &c.,
Lecturer on Chemistry at the St. George's
School of Medicine.

G. P. GIRDWOOD,
Assistant-surgeon, Grenadier Guards.

Laboratory, St. George's School of Medicine, July, 1856.

NERVES AND GANGLIA OF THE UTERUS.

To the Editor of THE LANCET.

SIR,—The feeble and evasive reply of Dr. Robert Lee, in *THE LANCET* of the 12th ult., cannot be accepted as an answer to the questions which have been several times publicly addressed to him. Some further explanation is requisite than the statement of Dr. Lee—viz., “I have not been guilty of furtively destroying the structures which I had discovered, and to display which so much time and labour has been expended.” No one has accused Dr. Lee of destroying structures which have no existence, but he is accused of certain practices which can only be characterized as scientific frauds. Three of these I have particularized:—

1. He has created a deceptive appearance of continuity between the hypogastric nerves and the muscular system of the uterus, by paring, with scissors, the surface of the connecting cellular tissue, previously blanched and hardened by lengthened maceration in alcohol.

2. When this deceptive appearance of continuity was no longer required, as in the dissections of the virgin uterus, he has furtively removed the cellular tissue, apparently in order to make the nerves of the virgin uterus appear smaller than those of the gravid organ; and whilst doing this in private, has publicly declared the removal of this neurilemma “unwarrantable,” “unjustifiable,” “unprecedented,” and that dissections so treated become “mutilated specimens.”

3. He has described in his own handwriting, and caused to be figured, upwards of fifty ganglia on the surface of the heart, which have no existence in nature.

Each of these charges I am prepared to substantiate by reference to his own dissections and his own writings. It is therefore not sufficient for Dr. Lee to say, “I have not been guilty,” for by his own works it can be proved that he has put forth statements which cannot be substantiated, and that he has caused structures to be represented which do not exist in nature.

Perhaps you will allow me to offer a few words in explanation of the course I am now adopting. It is upwards of ten years since Dr. Lee began a system of misrepresentation with regard to my researches on the constitution of the sympathetic nervous system, and the nervous supply of the uterus and neighbouring organs; and during this long period, I have allowed these misrepresentations to pass almost unanswered, in the expectation that the author of them would cease to put them forth, and that the researches to which they referred would be finally estimated in accordance with any value they might possess. But in this I appear to have been mistaken; the misrepresentations continue, and even now, after so many

years, there does not appear any probability that they will cease. Under these circumstances, I feel forced to terminate this almost unceasing annoyance, and find I can only do so, with a chance of success, by exposing the practices which have been put in requisition to bolster up these assumed and much-vaunted discoveries. Any defence of my own researches would only have led to further misrepresentations and improper insinuations. The reluctance with which this course has been adopted, and the pain which this denunciation even now gives me, may be estimated from the fact, that I have borne a continued injurious misrepresentation for upwards of ten years rather than have recourse to it. I am, Sir, &c.,

Langham-place, July, 1856.

T. SNOW BECK.

To the Editor of THE LANCET.

SIR,—“Dr. Snow Beck has two specimens,” says Dr. Herschfeld, “in the preparation of which he employed eight months, removing from day to day, with forceps and needles, (as he told me himself,) not merely the cellular tissue, but the neurilemma even, so that he has left only the fasciculi of the nervous tubes. It seems to me that the work of this anatomist ought not to be appealed to, when the subject of the volume of the nerves of the uterus is agitated, for they only present the nerves deprived of one of their constituent parts—the neurilemma.”

I have read Dr. Beck's paper on the Nerves of the Uterus in the “Philosophical Transactions” for 1846, and have not been able to find the most remote allusion to the fact above stated, and now admitted to be correct. Will you permit me to inquire of him why this fact was not explicitly stated in the paper, and if it was made known to Dr. Todd, Dr. Sharpey, or Mr. Bowman, the individuals through whose exertions chiefly the Royal Medal was fraudulently awarded in 1845? I am, Sir, yours, &c.,

July, 1856.

INVESTIGATOR.

ABUSES AT THE LONDON HOSPITALS.

To the Editor of THE LANCET.

SIR,—Permit me to call attention, through the medium of *THE LANCET*, to what must be considered a most serious evil in one, at any rate of the London schools of medicine—viz., St. Bartholomew's. Whether the evil exists in other schools I know not, but that it does exist in St. Bartholomew's I do know, having been a student there for the last few years. The evil to which I allude is the great want of punctuality of some (I may venture to say, of most) of our physicians and surgeons in their daily visits round the hospital wards. Every student who has at all diligently attended the ward practice of this hospital must long ago have felt the serious inconvenience and loss of valuable time which he has had to put up with, from the inattention to punctuality of those whose ward practice he has been attending. In the *school*, the professors have stated times for the delivery of their lectures, and they observe these hours most scrupulously. In the *hospital*, also, the physicians and surgeons have hours fixed for them to visit the wards; but many of them observe these hours only so long as they have no private engagements to call them away. Now, many of these gentlemen, having large private practices, have many private engagements, and as they allow these engagements to interfere with their hospital visits, the consequence is, that their visits to the wards are made very irregularly, and clinical instruction is given in a most hurried and negligent manner.

I could mention one or two gentlemen at this hospital, who, while scrupulously punctual in the delivery of their professional lectures, pay their visits to the hospital wards pretty much as it suits their convenience. Now, Sir, is this just to the students attending their practice, who mostly pay pretty handsomely for that privilege? If, as is generally allowed, ward practice is by far the most important part of one's hospital career; and if punctuality is demanded (and obtained) in the daily delivery of lectures in the theatre, surely it should be insisted upon in the daily visits to the bedside.

What is the consequence of this evil and what is the remedy for it? The consequence is, that clinical instruction, from being irregularly and negligently given by the teachers is often totally neglected by the pupils. I remember not a few who gave up “going round” in disgust; finding day after day that the physician's or surgeon's visit was most uncertain, and that when he did make his appearance his only object was to hurry over the cases as rapidly as possible and be off again. When we compare the manner in which clinical instruction is given